OBITUARY NOTICES

OF

FELLOWS DECEASED.

CONTENTS.

SIR BENJAMIN BAKER, K.C.M.G., K.C.B., 1840—1907.

Sir Benjamin Baker was born on March 31, 1840, at Keyford, Frome, Somerset, and he died at Bowden Green, Pangbourne, Berkshire, on May 19, 1907. In a career, during the greater part of which he was associated with Sir John Fowler, K.C.M.G., he achieved the position of an engineer of the highest originality and distinction and was engaged in the design and construction of, or as responsible adviser for, a very great amount of civil engineering work of the most varied character. His connection with the Forth Bridge and the Assuan Dam alone are sufficient to mark him out as an engineer of the highest rank.

His parents were Benjamin Baker and Sarah Baker (*née* Hollis). His father appears to have come from Carlow, in Ireland, and was principal assistant at iron works at Tondu, Glamorgan. Sir Benjamin Baker was educated at the Cheltenham Grammar School. At the age of 16 he was articled to Mr. H. H. Price, of the Neath Abbey Iron Works, where some of Trevithick's pumping engines and some early locomotives and marine engines had been built. In 1860, he became assistant to Mr. W. Wilson, then in association with Mr. (afterwards Sir John) Fowler engaged in the erection of the Pimlico Railway and the Victoria Bridge and station. In 1861, he passed into the office of Mr. Fowler, where he was engaged in designing roofs, girders, and retaining walls for the Metropolitan Railway, the construction of which was then about to be commenced. He assisted in the preparation of the plans for the extension of the Metropolitan Railway and, in 1870, became Chief Assistant and junior partner to Mr. Fowler, having general charge of the construction of the Metropolitan and District Railways. His association with Sir John Fowler continued till the death of the latter in 1898.

As soon as he was engaged in Mr. Fowler's office, Baker set himself with zeal to the investigation of the mechanical problems suggested by the work on which he was engaged and the result of his studies appeared in contributions to 'Engineering,' in a series of papers on "Long Span Bridges," 1867, republished in England and America and translated and printed in Germany, Austria and Holland; "On the Strength of Beams and Columns," in 1868, also republished in 1870; "On the Strength of Brickwork," in 1872; and "On Urban Railways," in 1874.

In the articles on long span bridges, after examining the conditions for securing the greatest economy of material in the various types of girder then in use, for spans from 300 feet up to the limiting span possible, he arrived at the conclusion that, by a system of cantilevers, supporting an independent girder, an opening might be bridged which could not be spanned by any of the systems previously examined, even with an infinite amount of material. The

reasoning is throughout extremely original and instructive, and the conclusion reached was afterwards verified in the construction of the Forth Bridge. The thoroughness of the investigation was shown in the further examination of braced arch, stiffened suspension and suspended girder bridges, types as to which little experience had then been obtained. The advantage of some of these newer types in permitting erection without scaffolding is pointed out. In 1865, a project for a bridge over the Severn, with a span of 600 feet, was prepared in Mr. Fowler's office, in which it was proposed to erect successive bays by building out from each side of the main piers, carrying on the process till the two opposing halves met and formed a continuous structure. This was the system subsequently adopted at the Forth Bridge and in many bridges since.

In a revision of this treatise in 1873, a section was added on short span bridges. Baker drew attention to the fact that in short spans the action of the rolling load is the point above all others requiring attention. His experience had forced on him the conviction that the destructive action of a frequently recurring load, not small compared with the dead load, was at that time habitually underrated. This was one of the earliest recognitions in this country by a practical engineer of the law of fatigue which Wöhler had discovered.

It may also be mentioned in this connection that Baker, in 1887, contributed to the 'Transactions of the American Society of Mechanical Engineers' "Some Notes on the Working Stress of Iron and Steel." He pointed out that while in some bridges in which the ratio of dead to live load was large, stresses of 17,000 to 20,000 lbs. per sq. inch had proved to be safe, yet in small spans such stresses would quickly lead to destruction. Hence in the elevated railway of New York the stresses were limited to 8000 lbs. per sq. inch in the flanges and to 4500 in members subject to reversals of stress. He gave the results of a series of experiments similar to those of Wöhler on soft and hard steel and iron. These showed that the opinion of some engineers, that alternating stresses are destructive only if they exceed the elastic limit, is erroneous. He indicated that the resistance of riveted joints to slipping is due to frictional adherence and stated that in the Forth Bridge the stress on riveted joints was kept within the limit of adherence. He expressed further the opinion that both the old-fashioned Government regulations requiring a fixed working stress for all kinds of loading and modern formulæ based on Wöhler's results failed to meet the requirements of engineers.

In a further series of articles in 'Engineering,' in 1868, "On the Strength of Beams, Columns, and Arches," republished in 1870, Baker discussed a problem which troubled many engineers at that time, namely, that the ultimate strength of beams is widely different from the strength calculated on the assumption that the material is elastic up to rupture. He adopted as an explanation Barlow's theory, which would not now be accepted, the discrepancy being known to be due to plasticity. But Baker used a mass of practical data and derived his coefficients so that his results were approxi-

mately true and useful. He applied his investigation to the then important question of the relative strength and stiffness of different sections of rails.

Like all Baker's papers, these articles contained many experimental observations carefully and accurately made.

The first great work on which Baker was engaged in a position of responsibility was the construction of the London Underground Railways, and his connection with this work continued till the completion, in 1871, of the sections from Moorgate Street to the Mansion House, a length of 13 miles. He described the works, which were of a specially novel, difficult, and expensive character, in a paper in the 'Proc. Inst. Civil Engineers,' vol. 81, 1884—85, and discussed generally the problem of Urban Railways in some important papers in 'Engineering' in 1874. He pointed out in these articles that at the time of the inception of the system of underground urban railways, none of the engineers concerned, either as promoters or opponents, evinced the dimmest intuition of the fact that the traffic over an urban line might be the heaviest in the world, and of a character to test the capabilities of a locomotive engine to the uttermost. It was even proposed at first to work the trains with locomotives carrying a charge of hot water, and an engine of this type was built, with unsatisfactory results. It was this intention which led to an insufficient provision for ventilation, which afterwards gave much trouble. In portions of the line constructed later, the stations were in open cutting, and a length of open cutting was introduced between the stations. Baker pointed out the great expenditure of power in acceleration required with stations half a mile apart and suggested that an ideal urban railway should undulate, the stations being placed at the summit of the undulations. By this means gravity would assist the engine in starting and supplement the brakes in stopping. He was able to carry out this arrangement subsequently in the construction of the Central London Tube Railway. He indicated the necessity for great tractive force to ensure a reasonable mean speed and the need of powerful brakes, because the time occupied in accelerating and reducing speed is a large fraction of the whole time of transit when the stations are not far apart. He showed that the laws governing urban traffic were widely different from those obtaining on ordinary railways, and that with weak engines and inefficient brakes the horse-power would vary as the cube of the speed. He calculated that with a level line and moderate speed about 60 per cent. of the energy of the engine is expended in the mischievous work of grinding the brake blocks, and that of 36 lbs. of fuel used per train mile only 15 lbs. would be usefully employed. He checked his calculations by observations on the Metropolitan Railway, where, with the powerful engines used, the mean speed was only 12 miles an hour. He showed that with an undulating railway with the stations at the summits, 50 per cent. more speed could be obtained with the same fuel consumption as on the existing railway.

The building of shallow underground railways through the heart of a great city involved a host of new and unexpected problems in construction and

difficulties in dealing with the pipes, sewers, and other obstructions below the street surface, and in supporting, with as little damage as possible, the heavy buildings above the railway.

Baker was largely concerned in the introduction of electrically worked tube railways in London. He was Consulting Engineer to the South London, the first tube railway, and the still more important Central London Railway was constructed under his superintendence. This railway, of $6\frac{1}{2}$ miles in length, consists of two tunnels of circular section, built with a casing of cast-iron segments, 11 feet 6 inches in diameter. At the stations the cylinders are 21 feet 6 inches in diameter. The railway is generally about 60 feet below the street level, and few difficulties or obstructions were met with. In this railway the stations are at the summit of undulations, the gradients falling each way so that the arrangement suggested in the early papers on urban railways was for the first time carried out. The railway was commenced in 1896 and opened by the late King, then Prince of Wales, in 1900.

From the year 1869, Mr. Fowler was much engaged in Egypt in advising the Khedive Ismail Pasha in regard to various engineering projects for developing the resources of the country, and Baker made more than one visit to Egypt to assist his partner, and later became Consulting Engineer to the Egyptian Government. One result of studies then undertaken was the project for the Soudan Railway between Wady Halfa and Shendy near Khartoum and a ship incline at Assuan. By means of a railway 3 kiloms. in length, over which boats, floated in a cradle, could be dragged by hydraulic machinery of 400 h.p., the obstacle to navigation at the first cataract was to be overcome, and continuous navigation without change of boat established between Wady Halfa and Lower Egypt. From Wady Halfa a railway of 550 miles length and of 3 feet 6 inches gauge was to be constructed at a cost of £4,000,000, to tap the rich southern provinces; about 60 miles were constructed and then the financial difficulties of Egypt compelled the interruption of the work.

Another great project in Egypt in which Fowler and Baker were concerned, in 1875, was a Ship and Irrigation Canal (an alternative Suez Canal) *via* Cairo and Alexandria. The project embraced a sweet water ship canal, 118 miles in length, from Alexandria to Cairo, and another from Cairo to Suez, a distance of 122 miles. At Cairo, low water is 39 feet above sea level, so that there would be a current down the canals to the Mediterranean and Red Sea. The rate of the current would be manageable and would depend on the amount of water abstracted for irrigation. Locks were to be provided on both stretches of the canal. For crossing the Nile at Cairo a railway bridge was to be provided, connecting the lines on the two sides of the river, and serving to support a traversing mooring to which ships could be attached when crossing the river. It was estimated that the payments for irrigation water would give a handsome return on the expenditure, independent of ship dues. In 1883, when the question of doubling the Suez Canal was mooted, Baker and Fowler, in an article in the 'Nineteenth Century Magazine,' recalled attention

to the advantages of this project, not only as providing an alternative ship canal, but as a means of affording high level irrigation and reclaiming a large area of desert.

Sir B. Baker was, at various times, consulted about the repairs and additions to the Delta Barrage, erected originally by French engineers, which had proved incapable of sustaining the required head of water in consequence of the unsatisfactory foundation. After various partial repairs by Sir Colin Scott Moncrieff, Sir W. Willcocks, and Colonel J. H. Western, it has finally been rendered completely stable and satisfactory by Major Sir Hanbury Brown.

A result of Baker's visits to Egypt was a paper on the hydrology of the Nile,* dealing with the slope, variation of level and flow, and amount of solids carried, largely based on his own observations.

In 1875, Garibaldi, then at the zenith of his popularity, was urging the Italian Government to undertake the diversion of the Tiber, in order to prevent the flooding of Rome and the Campagna. The Government considered the cost of the project prohibitive, but it had to be treated seriously. Baker and Fowler were called into counsel. Some surveys were made, and finally more moderate plans of rectification and embankment were adopted.

In 1878, Baker designed a vessel to bring Cleopatra's Needle to this country. Messrs. John and Waynman Dixon first suggested the removal of the obelisk, Mr. Fowler and Lord Vivian obtained the Khedive's consent, and Mr. Erasmus Wilson offered a contribution of £10,000 for the purpose. While in the case of the Luxor Obelisk, at Paris, the removal occupied seven years, Cleopatra's Needle was erected on the Embankment 18 months after the order to build the vessel was given. Baker designed a vessel of circular section, in which the needle was rolled into the water and towed to Alexandria. There final arrangements were made, and thence the vessel was towed to England. Unfortunately, in a storm, some rails used as ballast broke loose, and the crew in a panic abandoned the vessel. This accident was due to an oversight, but the vessel was never in real danger. She was found, towed to Ferrol, and then to England. The Needle was fitted with trunnions, lifted in a horizontal position, and then swung to a vertical position, the operation being carried out with the greatest ease.

Following some earlier abortive proposals, the Forth Bridge Company was formed in 1873, to erect a suspension bridge with spans of 1600 feet, designed by Sir T. Bouch. But the failure of the first Tay Bridge, also designed by Sir T. Bouch, led to reconsideration of the plans, the suspension principle was abandoned, and a design for a steel cantilever and central girder bridge, with spans of 1710 feet, submitted by Messrs. Fowler and Baker, was adopted. The construction of this great bridge involved the co-operation of many distinguished engineers and contractors, and its successful completion is an achievement in the honour of which they all share. But, no doubt, the

* 'Proc. Inst. Civil Engineers,' vol. 60, 1879—80.

general design is due to Baker, who carried out for the first time the previsions of his early treatise on long span bridges. Sir John Fowler and Sir B. Baker also kept a personal and continuous control over the entire operation of building the bridge. The contract was let in December, 1882, and the opening ceremony took place, under the auspices of the Prince of Wales, on March 4, 1890. This is not the place to enter upon a description of this immense work. Baker gave an account of the bridge in a paper at the British Association in 1882, and at the meeting at Montreal in 1884; also in his Presidential Address at the Mechanical Section of the British Association in 1885, and in papers at the Iron and Steel Institute in 1885, and at the Royal Institution in 1887. Reference may also be made to the admirable record on the Forth Bridge reprinted from 'Engineering' in 1890, and to "Die Forth Brücke," von G. Barkhausen, 1889. An exceedingly important and troublesome question in designing the Forth Bridge was the provision to be made for wind pressure. The failure of the first Tay Bridge was due, at any rate to a great extent, to the lateral pressure of the wind, and subsequently, perhaps, excessive values had been assumed for the intensity of wind pressure. In the case of the Forth Bridge, the immense area exposed made the question of wind pressure a governing consideration in design. The maximum wind pressure in accordance with the Board of Trade rule was assumed as 56 lb. per square foot, acting on twice the vertical projection of one side of the bridge. But to remove doubts as to the adequacy of this provision, experiments were made. A wind gauge of 300 square feet area was erected on the island of Inchgarvie, with small comparison gauges. Some account of these experiments was given in the Montreal paper on the Forth Bridge, and in the 'Proc. Inst. Civil Engineers,' vol. 69, p. 145, and vol. 156, p. 119. They satisfied Baker that the assumed pressure was in excess of anything likely to be realised. Further experiments were made on the shielding effect of one surface on another surface behind it. A suspended cross bar carried an adjustable flat surface at one end and a model of any structure of more complex form at the other. By oscillating this simple apparatus and adjusting the plane surface, the plane area equivalent in resistance to the more complex model was found. The results were very interesting and valuable.

Baker had had great experience in the use of steel, and had made very many experiments on its behaviour under straining action. His confidence in it was great, and in designing the Forth Bridge he ventured to use, for the compression members, steel of higher tenacity than had previously been adopted in structures. He thus anticipated the tendency to use high tensile steel, which is now not uncommon in suitable cases.

In 1881, Sir Benjamin Baker contributed a paper to the 'Proceedings of the Institution of Civil Engineers,' on the "Lateral Pressure of Earthwork." He pointed out the deficiency of experimental investigation, and criticised adversely the theories of earth pressure on which engineers chiefly relied. The paper contains a mass of instructive observations on the pressure of

earth and the failures of retaining walls. The paper gave rise to a very interesting discussion, and to a communication from the veteran mathematician, Boussinesq. A very characteristic statement from Baker's reply to the discussion may be quoted:—"He protested against the charge implied against him of a contempt for theory. His habit of thought and mode of working were entirely opposed to such a feeling, and indeed, in his opinion, an engineer who did not attempt to classify his practical data, with the ultimate aim of elucidating a satisfactory theory, was wilfully playing the part of a blind man." Another important practical paper, based on a very large experience, was one on "Railway Springs."*

Egypt is a country nearly rainless, and dependent on the Nile for its water supply. Hence, irrigation from the river has been practised from a great antiquity. But the water supply is insufficient in the summer months for perennial irrigation in middle and lower Egypt, and the level of the river is too low to adequately feed the canals. The satisfactory repair of the Delta Barrage made the question of increasing the flow at low Nile a very urgent one. In 1889, Colonel J. W. Western, R.E., began an investigation of projects for storing water in the winter months to increase the river flow in summer. On his retirement, Sir W. Willcocks, who was appointed Director of Reservoirs, continued the study and prepared three schemes, one for a reservoir in the Wady Rayyan, two for reservoirs in the Nile Valley, near the First Cataract. Generally, the scheme for a reservoir near Assuan was favoured, the reservoir being formed by constructing a masonry dam across the river. It was shown that if such a scheme were carried out, not only could the supply of the existing irrigation canals be ensured at all times, but a large increased area of land could be brought into profitable cultivation. In flood, the River Nile carries so much silt that water then impounded would gradually but certainly fill up a reservoir with deposit. It was necessary, therefore, that a dam should be so constructed as to allow the silt-bearing flood water to pass through, and to impound water only when the river flowed clear. Sir W. Garstin, Secretary to the Public Works Department, generally endorsed the views of Sir W. Willcocks. At his suggestion, an International Commission of distinguished engineers was then appointed by Lord Cromer to report on the plans, consisting of Sir B. Baker (England), Mr. Giacomo Torricelli (Italy), and M. Auguste Boulé (France). The two former reported adversely to the other plans, but favourably to the scheme of a reservoir at Assuan, suggested some modifications of the designs of Mr. Willcocks, and selected a site for the dam. M. Boulé reported separately, dissenting from the views of his colleagues, not on the ground of any doubt as to the practicability of the scheme proposed from an engineering point of view, but from an objection to any interference with the temples at Philæ, which, on the scheme recommended, would be partially submerged. As to the absolute need of a reservoir, no doubt was expressed

* 'Proc. Inst. Civil Engineers,' vol. 66.

by any member of the Commission. It was estimated that its construction would increase the revenue of the State by £750,000 annually, and would result in benefit to cultivators of ten times that amount. Baker suggested that, as a last alternative, the temples at Philæ could, if necessary, be raised 40 feet at an expenditure of £200,000.

In the plans of Sir W. Willcocks, the height of the dam was to be 85 feet, and the reservoir capacity 88,300 million cubic feet. To meet objection to the submersion of Philæ, the height of the dam was reduced to 65 feet, and the reservoir capacity to 37,612 million cubic feet.

In 1898, Sir Ernest Cassel entered into financial arrangements with the Government, taking bonds repayable in 30 years, and engaging to supply the funds necessary during the progress of the undertaking. A contract was signed with Messrs. John Aird and Company, and Sir B. Baker was appointed Consulting Engineer. The dam consists of two parts, one 4600 feet in length, pierced by 180 sluices at four levels, the other 1800 feet long and solid. A lock and canal makes the passage of the cataract easy to steamers at all times, thus making the Nile continuously navigable up to the Second Cataract at Wady Halfa. The work was carried out successfully, and completed in 1902, in less than the contract time. The Assuan Reservoir extends to Ibrim, a distance of 140 miles from the dam.

On the plan carried out, the water-level, with reservoir full, rose to the floor of the Philæ temples, then situated on an island in the reservoir. It was found that parts of the foundations of these temples were on silt and in a bad state, and likely to be further damaged by the action of the water. Underpinning with steel girders surrounded with cement on an extensive scale was carried out with considerable difficulty and at great cost, and the stability of the masonry of the temples was secured.

A subsidiary work was simultaneously executed at Asyût, 339 miles below Assuan and 246 miles above Cairo. From this point a large area is irrigated by the Ibrahimia Canal, which was with difficulty supplied during the summer months. By the construction of a dam across the Nile a permanent supply could be ensured, and with the larger flow in the river due to the Assuan Reservoir, a considerably increased area in Middle Egypt could be placed under perennial irrigation. The original plans were prepared by Sir W. Willcocks, Sir B. Baker was appointed Consulting Engineer, and the work was carried out by Messrs. Aird. The river is 2953 feet in width, and the dam is an arched viaduct, founded on a masonry floor, with sluices in the openings. The urgent importance of an early completion of the work being realised by Sir B. Baker, he advised that the contract should be cancelled, the work pushed on regardless of cost, and the question of profit to the contractors left to himself. Lord Cromer and the contractors agreed to these terms, the work was finished a year under the contract time, and the Public Works Department admitted that £600,000 had been saved to the country owing to the extra year's supply of water.

In 1902, Sir Benjamin Baker gave a lecture on the Nile dams at the Royal Institution, at which the Prince and Princess of Wales were present. He contributed an article on the "Nile Reservoirs and Philæ" to the 'Nineteenth Century' magazine in 1894. (Reference may also be made to papers by Mr. Maurice Fitzmaurice, C.M.G., "On the Nile Reservoir, Assuan," 'Proc. Inst. Civil Engineers,' vol. 152, p. 71; by Mr. F. W. S. Stokes, "On the Sluices and Lock Gates of the Nile Reservoir, Assuan"; and by G. H. Stephens, C.M.G., "On the Barrage at Asyût," 'Proc. Inst. Civil Engineers,' vol. 158, p. 26.)

The success of an immense work of this kind must depend on the energy, the ability, and the resourcefulness of a great number of persons, and, in speaking of it at the Institution of Civil Engineers, Sir B. Baker gave unstinted praise to his colleagues in Egypt who carried out the operations in a trying climate. But undoubtedly the reliance placed by Lord Cromer and those in authority on Sir Benjamin Baker's experience and judgment was an important factor in undertaking the work; he spent time every winter on the works; he provided beforehand, by careful foresight and consideration, for difficulties which might arise, and his wise direction, resourcefulness, and courage were essential elements in the success achieved.

The construction of the Assuan Reservoir proved of immediate and enormous advantage to the prosperity of Egypt, and it very soon became evident that a still larger supply of irrigation water was necessary. By 1905, the whole of the water stored at Assuan was appropriated, though a vast area of land was still left without water, and the increase in the value and productivity of the irrigated land exceeded expectations.

A site for a second reservoir above Assuan was sought, but no suitable position could be found. Finally, it was decided to raise the dam at Assuan 7 metres, or about to the height at first contemplated by Sir W. Willcocks. It is a matter of regret that this will involve the partial submergence of the Philæ temples during part of the year. But it can at least be said that this was not decided on till every alternative had been examined and unavoidably rejected. The addition of new to old masonry in a work which has to resist water pressure, and in a country where the temperature changes are great, is a matter of considerable difficulty. Sir Benjamin Baker considered long and anxiously the method of proceeding, and under his direction some very interesting experiments were carried out on model dams to elucidate the distribution of stress. Ultimately he developed a plan for strengthening and raising the dam by constructing an independent mass of masonry free to settle and contract, after which it will be bonded to the older mass by cement grouting.

Shortly before his death he went to Egypt, and there the plans were decided on and the contract settled. Before long, the dam will be increased in height so that the storage capacity of the reservoir will be increased two and a-half times.

It is not needful in this notice to enumerate Sir Benjamin Baker's

professional works, but amongst the more important the following may be mentioned: He was, with Sir John Fowler, Chief Engineer for the remarkable Chignecto Ship Railway, which was commenced, but the works were stopped by the failure of the contractor, and finally abandoned by the Canadian Government. Jointly with Sir John Wolfe Barry, he was Consulting Engineer for the Avonmouth Docks; Engineer for the electrically-operated bascule bridges over the Swale on the South Eastern Railway and at Walney at Barrow-in-Furness; Consulting Engineer to the Public Works Department of Cape Colony, and responsible for the bridges erected there; Consulting Engineer, jointly with Mr. Shelford, for the West African Railways. He was called into council when the boring of the Hudson River Tunnel at New York seemed likely to be a failure, and designed a special form of shield by means of which the work was carried on. Jointly with Dr. Deacon he reported on the schemes for the supply of water to London from Wales.

Sir Benjamin Baker was a member of the Light Railways Commission of the Board of Trade; and of a Committee which reported to the Board of Trade in 1900, on the loss of strength in steel rails due to prolonged use, a Committee appointed after the serious accident at St. Neots due to a fractured rail. He was a member of the Standards Committee, instituted at the suggestion of the Institution of Civil Engineers, a Committee which is engaged on the large and important work of establishing standard forms, tests and specifications for all the materials used by engineers and standard types for locomotives and electrical machinery. In 1888, he was appointed a member of the Ordnance Committee at Woolwich and was senior civil member at the time of his death. This Committee, consisting of military, naval, and civil members, decides on all questions as to design, material, etc., of the war material manufactured in the Arsenal and small arms factories of the Government, and to his duties on it Baker gave unremitting attention.

He was member of a Committee appointed to consider the interference with the work of Greenwich Observatory due to the London County Council generating station on the bank of the river immediately below the Observatory. He was a member of the Executive Committee of the National Physical Observatory.

Sir B. Baker was often called in to advise as to the safety of structures which, erected at an earlier period, exhibited signs of decay, and to suggest means of reparation. Thus he reported on the condition of three of Telford's principal bridges, the Buildwas cast-iron arch bridge, the Over masonry arch bridge over the Severn near Gloucester, and the Menai suspension bridge. He succeeded in restraining the local authorities from pulling down two of these or doing anything which would affect their appearance. In the case of the Menai Bridge, he reported that the main chains were sound and that though the suspending rods had suffered from corrosion they would last till the present timber floor required renewal. When this became necessary he recommended the substitution of a steel floor and the repair of the suspending rods. He reported to the Dean and Chapter on the stability of St. Paul's

Cathedral. When part of the roof of Charing Cross Station fell, he made an immediate examination at some risk and on his advice the whole roof was reconstructed and the similar roof at Cannon Street Station strengthened.

Sir Benjamin Baker was the recipient of many distinctions and took an active part in many scientific societies. At the opening of the Forth Bridge he received the decoration of K.C.M.G., and for services at Assuan the K.C.B. and the order of Medjidieh of the First Class. He received the honorary degree of D.Sc. at Cambridge, that of LL.D. at Edinburgh, and that of M.Eng. at Dublin. The French Academy of Sciences awarded to him and to Sir John Fowler the Poncelet prize. He became Fellow of the Royal Society in 1890, member of its Council in 1892—3, and was one of its Vice-Presidents from 1906 till his death. At the Institution of Civil Engineers he became an associate in 1867, member in 1877, member of Council in 1882, and was President in 1895. He became member of the Institution of Mechanical Engineers in 1890, of its Council in 1899. He was on the Council of the Society of Arts from 1888 and took an active part in its affairs, also member of the Iron and Steel Institute, and hon. A.R.I.B.A. and A.I.N.A. He was made an honorary member of the American Society of Civil Engineers in 1897, of the American Society of Mechanical Engineers in 1886, and of the Canadian Society of Civil Engineers in 1888.

Sir Benjamin Baker was always very modest in speaking of his own part in undertakings for which he was responsible, and very generous in acknowledgment of the help he received from colleagues. He was always very ready to discuss with others the difficulties which arose from time to time, and he treated opinions put before him with much consideration, though always forming an independent judgment. He was actively generous in helping younger engineers, and for those who served him he long retained his goodwill, and often continued to correspond with them for years. He attended very closely to the business of numerous councils of which he was a member, and his judgment on the matters which arose was rapid, tolerant, and sagacious, and always carried great weight.

W. C. U.

EDWARD JOHN ROUTH,* 1831—1907.

By the death of Dr. Routh on June 7, after a period of gradually failing health, a commanding figure in the recent history of English mathematics has been removed. Born at Quebec in 1831, the son of a distinguished British officer, he was educated in London at University College School, and subsequently studied mathematics under de Morgan at University College. He matriculated at Peterhouse in 1850, but did not drop his London connection, obtaining the gold medal in mathematics with the degree of Master of Arts in 1853, then a somewhat rare distinction. At Peterhouse he had Clerk Maxwell, who soon after migrated to Trinity, as his rival in the same year; while Tait and Steele were undergraduates of the College, and Lord Kelvin (already Prof. W. Thomson, of Glasgow) was a junior Fellow.

Not long after taking his degree—in January, 1854, being Senior Wrangler, and bracketed with Clerk Maxwell for the Smith's prizes—he began the career of tuition of advanced honour men in mathematics, which was soon to lead to a unique reputation as a successful teacher. From 1858 to 1888 he had, in all, between 600 and 650 pupils, of whom the great majority graduated as Wranglers, twenty-seven being Seniors, while forty-one were Smith's prizemen; between 1861 and 1885, when he retired from this strenuous work at the age of 54, he had all the Senior Wranglers as pupils, with but one exception near the end of the time.† The number of his pupils, which was for many years about 100, was not at all unprecedented; what was unique was the fact that for all this time he directed, almost without challenge, most of the intellectual activity of the *élite* of the undergraduate mathematical side of the University. This herculean task naturally demanded methodical arrangements, and the husbanding of his resources to the utmost. What he aimed at was to impart thorough mastery of the main principles of ascertained knowledge over the field of mathematics then cultivated at Cambridge; it was clearly out of the question to stray very far into the regions of nascent science, in which ordered theory gradually evolves itself in response to concentrated and specialised effort. He was in the habit of claiming that this would follow spontaneously in the case of the mathematician born, once he had learnt mastery of the resources of the science, while even when it did not follow, the record in the legal and other professions of persons who had done well in youth in mathematical studies proved their supreme value as a deductive mental discipline.

His plan was to take small classes, each of about ten men selected to run together, and to maintain an average by catechetical methods. Those

* Reprinted from 'Nature,' June 27, 1907.

† These and other facts have been taken from a valuable notice in the 'Cambridge Review,' signed W. W. R. B.

who could go faster than the average had extra material provided in the form of manuscript digests for study, and especially in the institution of a weekly paper of about a dozen problems, selected from recent examination papers, or abstracted from memoirs in the home and foreign mathematical journals. An element of competition formed a stimulus in answering these papers, while written solutions were afterwards at hand for study in cases of failure to unravel them. Looking back on those times, it might be thought that there was too much problem and too little sustained theory; but no one ever accused the standard of the problems selected of being lower than it ought to be, while, on the other hand, absence of some such rigid procedure would have rendered quite impossible that focussing of undergraduate mathematical activity and ambition in one place, which was a main feature of the system. Men with further ambitions would struggle with Thomson and Tait's "Natural Philosophy" or with Maxwell's "Electricity," or with brilliant and stimulating courses of lectures given on growing special subjects by the more eminent mathematical physicists, and thus learn that though in youth mastery may be rapid, yet at all times invention must be slow. It was, moreover, thus possible for the abler men to have time to spare, to expand their outlook by taking up some other branch of knowledge as a relaxation from mathematics, or for joining in other activities of the University. Nowadays the field covered by the mathematical instruction offered at Cambridge is vastly wider than would have been conceived as practicable twenty years ago; but the question is still unsettled how far it is expedient to extend the preliminary undergraduate course into complex special theories.

Whatever may be thought as regards Dr. Routh's views on postponing special research in favour of thorough preparation, it could not be urged that he did not himself, notwithstanding his other absorbing work, set an example of what research might be. Many of his earlier papers, mainly in the 'Quarterly Journal of Mathematics,' related to the dynamics of rigid solids, spinning tops, rolling globes, precession and nutation, and such like; they were distinguished by the development of methods relating to moving systems of co-ordinate axes, and to the differentiation of vectors such as velocity and momentum with regard to them. In another connection he applied the kinematics of special systems of co-ordinate axes, moving along a curve, to problems of curvature and torsion. The advantages of these methods in differential geometry have come again into recognition, as may be seen in such works as Darboux's "Théorie des Surfaces." Afterwards, arising out of his researches on dynamical stability, which will be referred to presently in more detail, there came a series of papers in the 'Proceedings of the London Mathematical Society,' on the propagation of waves, and the analysis of complex vibrations in networks of interlacing threads, and in other such laminar systems, leading up to a mechanical treatment or illustration of the broad general theory of harmonic analysis, principal periods, and related topics.

In the early 'seventies, the question of the possible explanation of steady, including apparently statical, relations of material systems by the existence of latent steady motions, such as the rotations of concealed fly-wheels or gyrostats attached to the system, was much to the fore. The fundamental problem as regards such representations is their degree of permanence; for a state of motion which falls away, however slowly, cannot be appealed to in elucidation of secular steadiness of relations. At a later stage the ideas of the subject were crystallised by Lord Kelvin in his British Association address, Montreal, 1884, entitled "Steps towards a Kinetic Theory of Matter," and in later addresses on cognate topics, mainly reprinted in vol. i. (Constitution of Matter) of his "Popular Lectures and Addresses," culminating in a way in 1897 in his gyrostatic model of a rotationally elastic optical æther.

It is thus not surprising that the Adams prize subject at Cambridge for the period 1875–7, announced over the signatures of Challis, Clerk Maxwell, and Stokes, should have been the search for "The Criterion of Dynamical Stability." This subject suited Routh's predilections exactly; and his classical essay, "A Treatise on the Stability of a Given State of Motion, particularly Steady Motion," composed, as he states in the preface, almost entirely during the year 1876, was the result. The greater part of the work in the essay is analytical, and is concerned with the discussion of the nature of the roots of the algebraic equation determining the free periods of slight vibration of the dynamical system; but where it enters upon the discussion of dynamical principles, such as the criteria connected with the Energy and the Action, the essay moves in a high plane. In particular, the burning question of how adequately to represent latent, and therefore unknown, steady motions, such as those of concealed fly-wheels or gyrostats attached to the system, is solved at a stroke by the famous theorem of the "modified Lagrangian function." It was established, in fact, that the presence of concealed steady motions does not fundamentally alter the standard mode of analytical specification of dynamical interaction developed originally by Lagrange, except in the one respect that the effective Lagrangian function now involves terms linear in the velocity-components as well as quadratic terms. The procedure of Lagrange, evolved originally from the side of the Principle of Action, constituted the science of general dynamics by eliminating from the problem all variables the values of which are prescribed in terms of the remaining ones by relations of permanent constraint, thus reducing the dynamical analysis to the discussion of just as many quantities as are required to specify the state of the system. It gives cause for some surprise that nearly a century elapsed before the correlative step was taken, namely, the elimination, from the analytical specification of the system, of permanently steady or cyclic motions, as well as the permanent geometrical constraints above mentioned. In the hands of the analysts who treated the subject meanwhile, the requirements of the actual planetary and lunar theories were perhaps the main aim; it is only recently, and largely

in the hands of the English school, notably Lord Kelvin and Clerk Maxwell, in later conjunction with Helmholtz, and building largely on the earlier work of W. Rowan Hamilton, that the subject of general dynamics has been welded into an instrument for the inductive, and in many cases speculative, exploration of physical processes in general. Anyhow, it will be evident how fundamental an advance in the principles of the dynamical interpretation of nature was involved in Routh's formulation of what he called the "modified Lagrangian function."

The problem thus solved by Routh with remarkable simplicity had already been some time in evidence. In the first edition of Thomson and Tait's "Natural Philosophy" in 1868, the equations of Lagrange had been applied in most effective manner to problems of motions of solids in fluid media, the energy function involved being determined in terms of the motions of the solids alone, and the fluid thus being *ignored* in the subsequent work. This procedure was soon challenged by Kirchhoff, as going beyond the existing conditions of validity of general dynamical theory; a special justification for the case of motion in fluids was given by him, on the basis of a Least Action analysis, and a brief statement of it was included by the author in the German translation of the treatise. Soon afterwards the same difficulty was pressed on Lord Kelvin independently by J. Purser, who also published a justification on more physical lines. This was, not unlikely, the origin of Lord Kelvin's general theory of "ignoration of co-ordinates," first published in 1879 in the second edition of Thomson and Tait's treatise, but which probably existed in manuscript anterior to Routh's essay. A report was once current that most of it was worked out in the harbour of Cherbourg, while his yacht was refitting, and the carpenters were all the time hammering overhead. This form of the theory, though more expressly suggested by the needs of physical dynamics, was less complete in one respect than Routh's, in that it did not bring the matter into direct relation with a single characteristic function (Lagrangian function of Routh, kinetic potential of Helmholtz), but simply obtained and illustrated the equations of motion that arose from the elimination of the cyclic co-ordinates that could be thus ignored.

Later still, Helmholtz, in his studies on monocyclic and polycyclic kinetic systems, which began in 1884, and culminated in the important memoir on the physical meaning of the Principle of Least Action in vol. c. (1886) of 'Crelle's Journal,' developed the same theory more in Routh's manner, and built round it an extensive discussion of physical phenomena, so that on the Continent the whole subject is usually coupled with his name. Shortly before, the work of Routh and Kelvin had already been co-ordinated with the Principle of Action by more than one writer in England.

The most elaborate published result of Dr. Routh's scientific activity was the "Treatise on the Dynamics of a System of Rigid Bodies," which began as a thorough, though rather difficult, handbook in one octavo volume, but expanded in successive editions in a manner of which other classical instances

readily occur to mind, until it became a sort of cyclopædia of the dynamical section of theoretical physics. In the course of an inquiry some ten years ago as to the reason why English mathematical physicists had so much practical command over the application of their knowledge, the mode of teaching in Cambridge came under review; and in particular this book was discovered by Prof. F. Klein, of Göttingen, who made arrangements for its introduction to the Continental public in a German translation, containing some brief valuable annotations such as the wide analytical outlook at Göttingen suggested. Especially was emphasis given to the great extension of the scope of abstract dynamics above described, with which Routh's name was associated, it is to be hoped permanently. Somehow the book does not seems to have attracted even yet much sustained attention in France.

Until lately, Dr. Routh's presence was a familiar and welcome one to residents in Cambridge. Though he never sought public positions, his services were in requisition in many ways, as Senator and Fellow of the University of London, as member of the University Council at Cambridge, member of Council of the Royal Society, and in other activities; while he declined more prominent offices more than once. In society he was bright and attractive, though somewhat retiring, simple, and entirely free from any suggestion of superiority. The respect and affection which he inspired in a long succession of distinguished pupils found expression on the occasion of his partial withdrawal from work in 1888, when at a remarkable gathering of judges, engineers, and men of science, his portrait by Herkomer was presented to Mrs. Routh, with many expressions of warm appreciation. His leisure he employed mainly in mathematical research, and in the preparation of a series of treatises on subjects of mathematical physics, of which the only criticism to be made is that his wealth of valuable material tended to convert them into cyclopædias rather than text-books. His last public action was to take the lead in opposition to the proposals for change in the system of the Mathematical Tripos at Cambridge. It is possible that he did not fully realise the altered circumstances of the time, and the insistent claims of other studies; anyhow, it will be matter for congratulation if the new arrangements work as well and as smoothly as did the older Mathematical Tripos during the long period when the practical direction was mainly in his hands.

J. L.

DMITRI IVANOVITCH MENDELÉEFF, 1834—1907.

The name of Mendeléeff has long been honoured by the Royal Society. Though not the first to recognise a relation between the properties of the elements and their atomic weights, he was unquestionably the first to apply the principles embodied in the statement of the "Periodic Law" to the settlement of atomic weights, to the prevision of previously unknown elements, and to the recognition of the true relations of different groups of elements to one another. In recognition of the importance of these generalisations and of the great knowledge and enthusiasm with which he laboured at the subject, the Royal Society awarded to him, in 1882, the Davy Medal, jointly with Prof. Lothar Meyer, in 1892 the Fellowship of the Society, and in 1905 the Copley Medal.

Dmitri Ivanovitch was the fourteenth child of his father, Ivan Pavlovitch Mendeléeff, Director of the Gymnasium at Tobolsk, in Siberia. His mother, Marie Dimitrievna, belonged to the old Russian family of Kornileff, long settled as manufacturers of paper and glass in the neighbourhood of Tobolsk, the glass works being situated at the village of Aremziansk. There can be no doubt that Dmitri Ivanovitch owed much of his intellectual activity to his mother, who was evidently a woman of considerable mental power and self-instructed beyond the range of ordinary female education of that period. This debt Mendeléeff acknowledges in the introduction to his great work on Solutions, which he dedicated to the memory of his mother in the following interesting lines: "This investigation is dedicated to the memory of a mother by her youngest offspring. She could only educate him by her own work, conducting a factory. She taught by example, corrected with love, and to devote him to science she left Siberia, spending her last resources and strength. When dying she said, 'Refrain from illusions, insist on work and not on words, search patiently divine and scientific truth.' She knew how often dialectical methods deceive, how much there is still to be learned, but how with the aid of science, without violence, with love but firmness, all superstition, untruth and error are removed, bringing in their stead the safety of discovered truth, freedom for further development, general welfare, and inward happiness. D. Mendeléeff regards as sacred a mother's dying words. October, 1887." How full of energy she was is shown by the fact that at her husband's death she continued to manage the glass works at Aremziansk.

At the age of fifteen, Dmitri Ivanovitch came from his far-off birthplace to Moscow in order to continue his education. A year later he entered the chief Pedagogic Institute in St. Petersburg, where, being associated with the University, he was able to devote himself chiefly to the physical sciences. At the end of this course he was appointed teacher in the Government school at Simferopol in the Crimea, and later at the gymnasium at Odessa.

In 1856 he returned to St. Petersburg, and at the early age of twenty-two he was appointed "privat-docent" at the University. At this time, like most young chemists, to judge by the titles of his published papers, he passed rapidly from one subject to another, but he soon found matter for serious thought and experiment in the physical properties of liquids, especially in their expansibility by heat.

In 1859, by permission of the Minister of Public Instruction, Mendeléeff proceeded to Heidelberg, where, in a private laboratory, he devoted himself to further study of the physical constants of chemical compounds, communicating some of his results to 'Liebig's Annalen' and to the French Academy. Returning to St. Petersburg in 1861 he secured his doctorate, and was appointed soon afterwards Professor of Chemistry in the Technological Institute. In 1866 he became Professor of General Chemistry in the University, Boutleroff at the same time holding the Chair of Organic Chemistry. He was frequently employed by the Government in connection with the investigation of questions of technical importance, and notably concerning the oil supplies of Baku and the Caspian; also in the department of weights and measures. The latter service brought him on several occasions to England, where his remarkable and distinguished figure was quite familiar in scientific circles. In 1904 he celebrated his seventieth birthday, on which occasion he received congratulatory addresses from the Chemical Society of London, and from many other scientific associations and academies with which he was connected.

Mendeléeff died on February 2 (N.S.), 1907, followed three days later by his colleague Menschutkin.

The chief scientific work of Mendeléeff may be roughly classified under several heads. As already mentioned, some of his earliest labours related to the determination of physical constants, especially the dilatation of liquids, which resulted later in the establishment of a simple general formula for the expansion of liquids between 0° and their boiling points. Later, he was led to discuss that theory of solutions which regards them as consisting of definite chemical compounds of the solvent with the solute, existing in a liquid state and more or less completely dissociated. This theory he supported by his own experiments, especially on mixtures of sulphuric acid and water and of alcohol and water. The densities of the latter have been estimated with very great accuracy, and from them he isolated two out of three assumed compounds represented by the formulæ (1) $C_2H_6O + 12H_2O$, (2) $C_2H_6O + 3H_2O$, and (3) $3C_2H_6O + H_2O$. In this connection it is interesting to recall the fact that Mendeléeff was a declared opponent of the doctrine of free ions in solutions of electrolytes.

A third subject to which he gave much attention was the nature and the sources of petroleum. After visiting the Caucasus, he went in 1876 to see the oil fields of Pennsylvania, and on his return communicated to the Russian Chemical Society a theory concerning the formation of hydrocarbons in the earth's crust. Rejecting the hypothesis that these compounds resulted

from the decomposition of organic remains, he assumed, on various grounds, that the interior of the earth must consist largely of metals, iron predominating. Such a view, in consideration of the relatively high mean density of the earth, was already familiar; but supposing metals such as iron and manganese saturated with carbon, Mendeléeff explained the production of hydrocarbons from these compounds by contact with water at a high temperature. The resultant hydrocarbons would distil from the lower into the more superficial layers of the earth's crust, leaving oxides of the metals behind.

The subject with which especially the name of Mendeléeff is indissolubly connected is the development of the Periodic Law. The several stages in the history of the recognition of relations between atomic weights and properties of elements extend over more than half a century. So soon as a sufficient number of atomic weights had been estimated with some approach to accuracy, by Berzelius and others, the hypothesis of Prout attracted attention, and down to the time of Stas was regarded with some favour. In 1829, Doebereiner pointed out the existence of triads of closely related elements, such as chlorine, bromine, iodine—lithium, sodium, potassium, in which the atomic weights are so related that the middle term of each series is nearly the arithmetical mean of the two extremes. Thirty years later Dumas drew attention to the close analogy observable in such series with homologous series of carbon compounds.

The first step toward the recognition of a periodic relation was taken in 1864—5 by John Newlands, and this was followed, soon afterwards, by a scheme of the known elements, arranged by Odling. But Newlands' attempt was very imperfect, as many of the elements were incorrectly placed, and no room was left for discovery of new elements. Odling, at the end of his article, refers to the probable existence of "some hitherto unrecognised general law."

The question being left in this condition, Mendeléeff communicated to the Russian Chemical Society, in March, 1869, a paper on "The Relations of the Properties to the Atomic Weights of the Elements." An abstract published in the 'Zeitschrift für Chemie' (vol. 5, p. 405) contains several obvious misprints; but, correcting these, the following literal translation serves to show that Mendeléeff had discovered this unrecognised law and perceived most of its important consequences:—

"When the elements are arranged in vertical columns according to increasing atomic weight, so that the horizontal lines contain analogous elements again according to increasing atomic weight, an arrangement results from which several general conclusions may be drawn. (Here follows the table of elements.)

"1. The elements, arranged according to magnitude of atomic weight, show a periodic change of properties.

"2. Chemically analogous elements have atomic weights, either in close agreement (Pt, Ir, Os), or increasing by equal amounts (K, Rb, Cs).

"3. The arrangement according to atomic weights corresponds with the *valency* of the elements and, to a certain extent, to the difference in chemical behaviour, *e.g.*, Li, Be, B, C, N, O, F.

"4. The elements most widely distributed in nature have small atomic weights, and all such elements are distinguished by their characteristic behaviour. They are thus typical, and the lightest element, hydrogen, is therefore rightly chosen as the typical unit of mass.

"5. The magnitude of the atomic weight determines the properties of the element, whence, in the study of compounds, regard is to be paid not only to the number and properties of the elements and their mutual action, but to the atomic weights of the elements. Hence the compounds of S and Te, Cl and I, show, beside many analogies, striking differences.

"6. The discovery of many *new* elements may be foreseen; for example, analogues of Si and Al, with atomic weights between 65 and 75.

"7. Some atomic weights will presumably suffer correction; for example, Te cannot have the atomic weight 128, but 123 to 126.

"8. From the table, new analogies become apparent. Thus, U appears as an analogue of Bo and Al, which is in harmony with experience."

Many years later, Mendeléeff found a difficulty in placing the elements of the argon group and radium, these substances having been discovered long subsequently to the formulation of the "periodic" scheme.

In an article written for the 'Russian Encyclopædia,' and abstracted into English ('Nature,' November, 1904), he later acknowledges the independent existence of these elements, and places the argon group in a column by themselves. The first place in the same column is assigned to the ether, which he assumed to be molecular in structure with a very small atomic weight.

How some of his earlier predictions have been verified by the discovery of gallium, of scandium, and of germanium, which correspond to Mendeléeff's theoretical elements, ekaluminium, ekaboron, and ekasilicon, is matter of common knowledge, and supplies a complete justification of the scheme. And though there are some outstanding difficulties about individual elements, the construction of this scheme and the enunciation of the periodic law as a principle applicable to the whole of the chemical elements constitute one of the most fertile conceptions in the whole range of modern chemistry.

W. A. T.

GEORGE GORE, 1826—1908.

George Gore was born in 1826 at Bristol, where his father had a small business as a cooper. Leaving school at thirteen, he began work as an errand boy. At seventeen he was apprenticed to a cooper and worked at that trade till he was twenty-one, meanwhile studying science and making what experiments he could in his small leisure. He was from the first keenly interested in electro-deposition, and probably it was through his desire to pursue this subject that in 1851 he came to Birmingham, already the chief centre of electroplate manufacture, and here he spent the rest of his life. He appears to have supported himself at first by practising medical galvanism, the apparatus for which he had already improved while at Bristol. Meanwhile he held classes on electroplating and on chemistry and thus began his long career as a teacher in Birmingham. Later he was appointed Science Master at King Edward's School, a post which he held for many years.

In 1854 he published the first of a series of papers on the electro-deposition of metals and soon gained a reputation which led manufacturers in Birmingham and elsewhere to bring their difficulties to him for solution, and from this time onwards he held a leading position in the town as a consulting chemist. Perhaps his most important work consisted in the help which he gave in the early days to the art of electroplating by his numerous discoveries, many of them the basis of present day practice. He wrote several text-books on the subject, which have been widely used here and abroad.

For a time he was chemist to a phosphorus works, and while in that position he discovered the method of bleaching phosphorus by chlorine, which is still in use.

His best known contribution to pure science is his investigation of the properties of anhydrous hydrofluoric acid, which he succeeded in preparing chemically pure. This work occupied him for several years, from 1860 onwards, and was followed by a research on the properties of silver fluoride. Among other researches were investigations on properties of liquid carbonic acid, on ammonia as a solvent of the alkaline metals, and on the thermo-electric action of metals and liquids.

An indefatigable and incessant experimenter, he made many minor discoveries. In 1854 he found that antimony deposited under certain conditions in which it contained a small quantity of antimony terchloride was an unstable form, so that when struck or rubbed or touched with a red hot wire it suddenly rose in temperature to over 300° C. and changed from a black lustrous body to a greyish powder. This form he termed "Explosive Antimony."

In 1858 he invented "Gore's Sphere," an interesting modification of the Trevelyan Bar experiment, in which a sphere is set rolling round a pair of

circular heated rails and continues to roll round. He further found that the sphere would roll round the rails without other heat than that supplied by an electric current passed from rail to rail through the sphere.

Another discovery was that if a current is passed through a solution of mercuric cyanide and caustic potash between two pools of mercury, a series of crispations appear on the negative pool and humming sounds are given out.

A more important discovery, made in 1868, was that there is a critical point in iron as it cools from a red heat. He found that as a red hot wire begins to cool it suddenly lengthens and then contracts again. He showed that there was no converse effect on raising the temperature, and that the effect on cooling was accompanied by a change in magnetic permeability.

Dr. Gore was an ardent advocate for the endowment of research, writing in its support at a time when its importance was not recognised as it is to-day. Among his publications is a volume on "the Scientific Bases of National Progress," in which he urged the value of scientific research to the welfare of the nation. His views were to some extent realised in the foundation, about 1880, of an "Institute of Scientific Research" by a few citizens of Birmingham. Here Dr. Gore was installed and here he worked for the remainder of his life.

Besides a volume on "the Art of Scientific Discovery," Dr. Gore occupied his later years, when he was no longer able to experiment so vigorously, in the composition of two works on "the Scientific Basis of Morality" and on "the New Scientific System of Morality." In these he treated of morals from a materialistic point of view, for which he might have found more sympathy fifty years earlier.

Dr. Gore was elected a Fellow of the Royal Society in 1865, and in 1877 he received the degree of LL.D. from the University of Edinburgh. In 1891 he was given a Civil List Pension in recognition of his contributions to science. He died on December 20, 1908, when nearly eighty-three years of age.

By his will his residuary estate was equally divided between the Royal Society and the Royal Institution for the purpose of assisting original scientific discovery. The share of the Royal Society, amounting to nearly £2,500, has been invested as "the Gore Fund."

J. H. P.

JULIUS THOMSEN, 1826—1909.

Hans Peter Jürgen Julius Thomsen, distinguished for his thermochemical investigations, was born in Copenhagen on February 16, 1826. He was educated at the church school of St. Peter in that city, and subsequently at the von Westens Institute. In 1843 he commenced his studies at the Polytechnic, and in 1846 graduated there in Applied Science, and became an assistant to Prof. E. A. Scharling. Of his earliest years comparatively little is known. Thomsen, always a reserved and taciturn man, talked little about himself even to his intimate friends—and least of all about the days of his youth. It was known to a few that these days had not been smooth. Those who were best informed were conscious that to these early struggles much of that dour and resolute nature which formed a distinguishing trait in his character was due.

In 1847 he became assistant to Forchhammer, and for a time supplemented his scanty income by teaching agricultural chemistry at the Polytechnic. In 1853 he obtained a travelling scholarship, and spent a year in visiting German and French laboratories. He probably owed this scholarship in great measure to his first contribution to the literature of chemistry, namely, his memoir, 'Bidrag til en Thermochemisk System' (contributions to a thermochemical system), communicated to the Royal Society of Sciences of Copenhagen in 1852, for which he received the silver medal of the Society and a sum of ten guineas to enable him to procure a more accurate apparatus. In this memoir he sought to develop the chemical side of the mechanical theory of heat, doubtless under the influence of Ludwig Augustus Colding, an engineer in the service of the Municipality of Copenhagen, and a pioneer, like Mayer, in the development of that theory. Indeed, the Danes now claim for Colding, who had made experiments on the relation between work and heat as far back as 1842, but whose labours were practically ignored by his contemporaries, the position which the Germans assign to Mayer (see Mach's 'Development of the Theory of Heat'; also Tait's 'Sketch of Thermodynamics,' 1868, § 33). In 1861 Thomsen further developed his ideas in a memoir on the "General Nature of Chemical Processes, and on a Theory of Affinity based thereon," published in the 'Transactions of the Danish Academy of Sciences.' In this paper he laid the foundations of the chief scientific work of his life.

In 1853 Thomsen patented a method of obtaining soda from cryolite, so-called "Greenland," or ice-spar, a naturally occurring fluoride of sodium and aluminium, $Al_2F_6,6NaF$, found largely, indeed, almost exclusively, in Greenland, and particularly at Ivigtut. In 1854 he obtained the exclusive right of mining for cryolite and of working up the mineral in Denmark for soda and alumina. Actual manufacturing operations were begun on a small scale in 1857, and in the following year Thomsen planned the present large

factory at Oeresund, near Copenhagen, which was opened on his thirty-fourth birthday. The importance of this industry to Denmark may be seen from the circumstance that during the fifty years of its existence the firm have paid the Danish Government nearly £300,000 for the concession. From the start Thomsen took a large share in the management of the Oeresund works, and by his energy, foresight, and skill placed the undertaking on a sound commercial basis.

Although Thomsen died a rich man, mainly as the result of the industry he created, in the outset of his career as a teacher and a technologist his means were very straitened. He came of poor parents, of no social position or influence, and they were unable to further his inclinations towards an academical career. In 1854 he applied unsuccessfully for a position as teacher of chemistry at the Military High School in Copenhagen. During three years—from 1856 to 1859—while still engaged in developing his cryolite process, he acted as an adjuster of weights and measures to the Municipality of Copenhagen. It was a poorly paid position, but it kept the wolf from the door. At about this period he betook himself to literature, and published a popular book on general subjects connected with physics and chemistry—somewhat in the style of Helmholtz's well-known lectures—entitled 'Travels in Scientific Regions,' which had a considerable measure of success. He was, however, not altogether unknown even at this time as an author, since in 1853 he had collaborated with his friend Colding in producing a memoir on the causes of the spread of cholera and on the methods of prevention, which attracted much attention at the time of its appearance.

In 1859, whilst engaged in the Oeresund factory, he again applied to the authorities for a position as teacher at the Military High School, and succeeded in obtaining an appointment to a lectureship in physics, which he held until 1866. During his tenure of this office he devised his polarisation battery, which received many awards at International Exhibitions and was used for a time in the Danish telegraph service.

In 1859–60 he was "vicarius" for Scharling at the University, and in 1865 became a teacher, and in the following year Professor of Chemistry and Director of the Chemical Laboratory, a position which he retained—active to the last—until 1901, when he retired in the seventy-fifth year of his age.

Before his connection with the University, he founded and edited, from 1862 to 1878, in association with his brother, August Thomsen, 'the Journal of Chemistry and Physics,' one of the principal organs of scientific literature in Denmark.

In 1863 he was elected a member of the Commission of Weights and Measures, and was instrumental in bringing about the adoption of the metric system and the assimilation of the Danish system to that of the Scandinavian Kingdom.

In 1883 Thomsen became Chancellor of the Polytechnic High School of Copenhagen—a position which he held for about nine years. During this period he entirely changed the character and spirit of the school, and stamped

it with the impress of his earnestness and industry. Under his direction, new buildings were erected and arranged in accordance with the best Continental and American models.

It was while occupying the position of Director of the Chemical Laboratory of the University that Thomsen executed the thermochemical investigations which constitute the experimental development of the ideas he had formulated in his memoir of 1861. The results of these inquiries were first made known in a series of papers published from 1869 to 1873 in the 'Transactions of the Royal Danish Society of Sciences,' and from 1873 onwards by the 'Journal für Praktische Chemie.' The papers were republished in collected form in four volumes (1882–1886) by a Leipzig house under the title of 'Thermochemische Untersuchungen.' A summary of this experimental labour, which extended over a third of a century, was subsequently prepared by Thomsen, and published in 1905 in Danish under the title of 'Thermokemiske Resultater.' A translation of this volume by Miss Katharine A. Burke, entitled 'Thermochemistry,' renders it readily accessible to English readers.

To go through this material in detail is impossible here. It may be stated generally that practically every simple inorganic process has been investigated calorimetrically by Thomsen, or can be calculated by means of the calorimetric data furnished by him. In the case of organic substances, data have been given for estimating the heat of combustion of a large number of compounds. All these estimations were made by Thomsen personally, according to a pre-arranged plan, and in systematic succession during a period of more than thirty years. They comprise more than 3500 calorimetrical estimations. It has been truly said that this work is unique in the chemical history of any country.

Among the results of Thomsen's thermochemical inquiries which have special value for physical chemistry is his investigation of the phenomena of neutralisation, in which he shows that the basicity of acids can be estimated thermochemically, and that it can in this way be proved whether or not a point of neutrality exists. His observation that the heat of neutralisation is the same for a long series of inorganic acids, such as hydrochloric acid, hydrobromic acid, hydriodic acid, chloric acid, nitric acid, etc., supports the theory of electrical ionisation, inasmuch as this requires that the heat of neutralisation of the strong acids must in all cases be independent of the nature of the acid, because the process of neutralisation for all of them is the combination of the ion of hydrogen in the acid with the ion of hydroxyl of the base to form water. These investigations also led to the important thermochemical result that the heat of neutralisation of acids (or the heat of their dissociation) cannot be considered as a measure of the strength of the acids.

Another important result is the proof by experiment of the connection which exists between the gradient of the heat-effect with the temperature and the specific heat of the reacting substances. The law of conservation of energy

requires the relation $dU/dT = C_1 - C_2$, where U is the heat-effect, T the temperature, and C_1 and C_2 are the heat capacities of the two systems before and after the reaction; and Thomsen showed by investigation of the heat of neutralisation, the heat of solution, and the heat of dilution, that this relation was satisfied, thus verifying the precision of his determinations. For the purpose of his inquiry, the specific heats of a large number of solutions of salts were estimated by an ingenious method, and with an exactness hitherto unattained.

Of no less importance are Thomsen's thermochemical investigations on the influence of concentration on chemical equilibrium. In the year 1867 Guldberg and Waage published their molecular theory of the chemical effect of mass. But they had only verified the theory to a small extent and in particularly simple cases. They had not investigated the complete homogeneous equilibrium, because at that time no method existed for its experimental investigation. Thomsen showed that the estimation could be made thermochemically. By allowing, for instance, an acid to act on a salt of another acid in an aqueous solution, the latter acid will be partly replaced by the first, which will form a salt. By mixing, for instance, a solution of sodium sulphate and nitric acid, there are formed sodium nitrate and sulphuric acid, but the process will not proceed to completion. If we have estimated the heat of neutralisation of the two acids with sodium hydroxide, the difference between these two heat-phenomena will give the amount of heat corresponding to the total decomposition of the sodium sulphate, and the heat found experimentally by mixing the two solutions will therefore show to what degree the transformation has taken place. It would be possible to estimate thermochemically the amount of the four substances in solution, and thereby, by varying the concentration or the proportion between the initial quantities of substances, to calculate whether the Guldberg-Waage theory on the effect of mass was confirmed in this case.

Thomsen applied this method to a large number of different acids and bases, and was thus enabled to prove agreement with the law of the influence of mass in all the cases which he examined. He found particularly that the proportion of the one acid which remained combined with the base was constant with mixtures of constant proportion. On this basis he propounded the term *avidity* for the tendency of the acid to unite with the base, and he verified that the avidity was independent of the concentration, and only to a small extent varied with the temperature. The idea of avidity has since acquired great utility, particularly since other and more exact methods for its estimation have been found. Concurrently with this, its meaning has been made clear by the theory of ionisation.

On the basis of these estimations, Thomsen drew up the first table, based on experiments, of the relative strength of the acids, and the numbers in this table have been found to agree with the results obtained by examining the electrical conductivity of the acids.

It is worth noting that Thomsen not only produced the experimental proof

of the correctness of the Guldberg-Waage theory of the effect of concentration soon after the appearance of this theory, but also that he was the first to acknowledge and adopt it. It is remarkable that this work of Thomsen received so little attention, although it appeared in a widely circulated German journal, and it was not until ten years later that the law of the effect of mass was generally recognised, as the result of the work of Ostwald and van't Hoff.

Although Thomsen's title to scientific fame rests mainly upon his thermochemical work, his interests extended beyond this particular department of physical chemistry. He worked on chloral hydrate, on selenic acid, on ammoniacal platinum compounds, and on glucinum platinum chloride, on iodic acid and periodic acid, on hydrogen peroxide, hypophosphorous acid, and hydrogenium. He early recognised the importance of Mendeléeff's great generalisation, and contributed to the abundant literature it produced. His paper of 1895, "On the Probability of the Existence of a Group of Inactive Elements," may be said to have foreshadowed the discovery of the congeners of argon. He pointed out that in a periodic function the change from negative to positive value, or the reverse, can only take place by a passage through zero or through infinity; in the first case, the change is gradual, and in the second case it is sudden. The first case corresponds with the gradual change in electrical character with rising atomic weight in the separate series of the periodic system, and the second case corresponds with a passage from one series to the next. It therefore appears that the passage from one series to the next in the periodic system should take place through an element which is electrically indifferent. The valency of such an element would be zero, and therefore in this respect also it would represent a transitional stage in the passage from the univalent electronegative elements of the seventh to the univalent electropositive elements of the first group. This indicates the possible existence of a group of inactive elements with the atomic weights 4, 20, 36, 84, 132, the first five numbers corresponding fairly closely with the atomic weights respectively of helium, neon, argon, krypton, and xenon ('Zeitsch. anorg. Chem.,' 1895, vol. 9, p. 283; 'Journ. Chem. Soc.,' 1896, vol. 70, II, p. 16). He subsequently made known the existence of helium in the red fluorite from Ivigtut.

As evidence of Thomsen's manipulative ability and his power of accurate work may be mentioned his determination of the atomic weights of oxygen and hydrogen, and incidentally of aluminium. For the atomic weight of hydrogen he obtained the value 1·00825 when O = 16, which is practically identical with that of Morley and Noyes. He further made most accurate estimations of the relative densities of these gases, and of the volumetric ratios in which they enter into the composition of water. His value for the atomic weight of aluminium is nearly identical with that adopted in the last Report of the International Committee on Atomic Weights.

Thomsen maintained his interest in thermochemical problems up to the end, and was a keen and clear-sighted critic of the work which appeared from time to time during the later years of his life. This interest

occasionally gave rise to controversy, and some of his latest papers were wholly polemical.

Thomsen was a pronounced atomist, and to him a chemical process was a change in the internal structure of a molecule, and the chief aim of chemistry was to investigate the laws which control the union of atoms and molecules during the chemical process. He considered that chemistry should be treated mathematically as a branch of rational mechanics. But no one insisted more strongly than he how little we really know of these questions. In summarising his theoretical ideas in the 'Thermokemische Resultater,' he says, "An almost impenetrable darkness hides from us the inner structure of molecules and the true nature of atoms. We know only the relative number of atoms within the molecule, their mass, and the existence of certain groups of atoms or radicals in the molecule, but with regard to the forces acting within the molecules and causing their formation or destruction, our knowledge is still exceedingly limited." He fully realised that his own work was only the foundation on which the future elucidation of these questions must rest. "He worked," says Brönsted, "in the conviction that what we somewhat vaguely call the affinity of the atoms—their interaction, their attraction, its varying effect, etc.—follows the general dynamical laws, and that, as he worded it, the principle that 'might is right' holds good in chemistry as in mechanics. On this foundation he hoped it might be possible to evolve the laws for the statics and dynamics of chemical phenomena, even although the inner nature of the action is unknown."

Thomsen's merits as an investigator received formal recognition from nearly every country in the civilised world. As far back as 1860 he was elected one of the thirty-five members of the Danish Royal Society of Sciences of Copenhagen, and from 1888 until his death he was its President. In 1876 he became an Honorary Foreign Member of the Chemical Society of London. On the occasion of the fourth centenary of the foundation of the University of Upsala (created in 1477) he received the degree of Doctor of Philosophy *honoris causa*. In 1879 he was made an honorary M.D. of the University of Copenhagen. Two years later he was made a Foreign Member of the Physiographical Society of Lund, and in 1888 he was elected a member of the Society of Science and Literature of Gothenburg. In 1885 he became a member of the Royal Society of Sciences of Upsala, and in 1886 of the Stockholm Academy of Sciences.

In 1883 he and Berthelot were together awarded the Davy Medal of the Royal Society—a fitting and impartial recognition on the part of the Society of the manner in which the two investigators, whose work not infrequently brought them into active opposition, had jointly and severally contributed to lay the foundations of thermochemistry.

In the same year Thomsen was made a member of the Accademia dei Lincei of Rome, and in the following year he was elected into the American Academy of Arts and Sciences in Boston, and of the Royal Academy of Sciences of Turin. In 1887 he was made a member of the Royal Belgian Academy.

In 1886–87, and again in 1891–92, he was Rector of the University of Copenhagen. In 1888 he became Commander of the Dannebrog, and in 1896, and on his seventieth birthday, he was made Grand Commander of the same order. On the same occasion the Danish chemists caused a gold medal to be struck in his honour. In 1902 he became a Privy Councillor (Geheime Konferenz raad). In the same year he was elected a Foreign Member of the Royal Society of London.

He died on February 13, 1908, and was buried on the eighty-third anniversary of his birth and on the jubilee of the opening of the Oeresund factory. His wife, Elmine Hansen—the daughter of a farmer on Langeland—predeceased him in 1890.

Thomsen played many parts in the intellectual, industrial, and social development of Denmark. To Europe in general he was mainly known as a distinguished man of science. By his fellow citizens he was further recognised as an educationist of high ideals, actuated by a strong common sense and a stern devotion to duty; as an able and sagacious administrator; as a successful technologist and the creator of an important and lucrative industry based upon his own discoveries; and as a man of forceful character, who brought his authority, skill, and knowledge of men and affairs to the service of the communal life of Copenhagen.

Thomsen was a municipal councillor of that city for more than a third of a century. He occupied a commanding position on the Council, and was invariably listened to with respect. The gas, water, and sewage works of Copenhagen are among the monuments to his civic activity. From 1882 up to the time of his death he was a member of the Harbour Board of the port. In these respects Thomsen sought to realise Priestley's ideal of the perfect man—that he should be a good citizen first and a man of science afterwards.

T. E. T.

WILLIAM JAMES RUSSELL, 1830—1909.

William James Russell was born on May 20, 1830, the son of a banker at Gloucester. His grandfather, William Russell, lived at Birmingham and was an intimate friend of Priestley. He suffered for this friendship by having his house burned to the ground by the Birmingham mob, in the Church and King Riots of 1791, two days after Priestley's house had met the same fate.

Young Russell, the subject of this notice, was educated at private schools, at Bristol and Birmingham, and entered University College, London, in 1847, where he studied chemistry under Thomas Graham (afterwards Master of the Mint) and Alexander Williamson. In 1851 he was appointed the first demonstrator of chemistry at the then newly-founded Owens College, and assisted Professor (afterwards Sir Edward) Frankland to plan and superintend the building of the original chemical laboratories of the College. In those days, every man who wished to train himself seriously as a chemist spent some time at a foreign University, mostly in Germany. Accordingly, after remaining at Manchester for two years, Russell went to Heidelberg, where he worked under Bunsen from the autumn of 1853 to that of 1855 and took his degree, Ph.D. After returning to England, he lectured for some time at the Midland Institute, Birmingham, but went back to University College, London, in 1857, as assistant to his former teacher, Williamson. At this time the methods of gas-analysis were receiving much attention from chemists in consequence of the precision given to them by Bunsen. This precision, however, was attainable only by the application of numerous corrections involving comparatively laborious calculations. It occurred to Williamson that these corrections might, in nearly all cases, be dispensed with if the pressure and temperature of the gas to be measured were made the same as those of a fixed quantity of air caused to occupy always the same volume. Under these conditions it is not necessary, for comparative measurements, to observe the actual temperature and pressure of the gas: the quantity of it is directly proportional to its volume without further correction. Russell joined Williamson in devising apparatus by means of which this idea could be applied practically with accuracy and convenience, and he continued to occupy himself in improving the details for several years.

Among the results of Russell's purely chemical work, we may mention his discovery of the precipitation of silver from an aqueous solution of silver nitrate by gaseous hydrogen, and the determination of the atomic weights of cobalt and nickel. By decomposing the oxides with hydrogen he obtained, in 1863, the values $Co = 29{\cdot}370$ and $Ni = 29{\cdot}369$, and six years later, by measuring the hydrogen evolved when the metals are dissolved in hydrochloric acid, he obtained $Co = 29{\cdot}88$ and $Ni = 29{\cdot}35$. The corresponding numbers given by the International Table of Atomic Weights for 1908 are $Co = 29{\cdot}5$

and Ni = 29·35 (the actual numbers of the International Table are here halved to make them comparable with Russell's units).

Dr. Russell was among the earliest investigators of the absorption spectra of what are commonly counted colourless liquids. He and Mr. Lapraik, who assisted him in experiments on this subject, remark: "We have been able to find but few liquids which in columns of 6 or 8 feet do not give absorption spectra." As examples of his acuteness in following up casual observations and his ingenuity and perseverance in varying their conditions, we may mention his experiments "On the action of certain metals and other bodies on a photographic plate in the dark" and "On the formation of definite figures by the deposition of dust." He traced the effects dealt with in the former of these investigations almost certainly to the formation of traces of peroxide of hydrogen. Those described in the latter paper were successfully explained by Mr. J. Aitken, F.R.S., who devised an ingenious method of observing the actual process of formation of the figures.

Russell was long engaged in teaching work. From 1860 to 1870 he was Professor of Natural Philosophy at Bedford College (London). From 1868 to 1870 he was lecturer on chemistry in the Medical School of St. Mary's Hospital and from 1870 to 1897 he held a similar appointment at St. Bartholomew's Hospital. Both here and at St. Mary's he succeeded the late Dr. Augustus Matthiessen, F.R.S. He was elected a Fellow of the Chemical Society in 1851; he became Secretary of the Society in 1873, Treasurer in 1875, retaining the office for fourteen years till, in 1889, he was elected President. During his presidency, in 1891, the Society celebrated the fiftieth anniversary of its foundation. He was an original member of the Institute of Chemistry, founded in 1877, and was President from 1894 to 1897.

Russell was for twenty-five years, 1878 to 1903, a member of the Council of Bedford College (London) and Chairman from 1887, and his attention and sound judgment contributed greatly to the prosperity of the College.

He was elected Fellow of the Royal Society in 1872; he served twice on the Council and was a Vice-President from 1897 to 1899. He presided over the Chemical Section of the British Association at the meeting at Bradford in 1873.

Personally, Dr. Russell was quiet but genial in manner, and he was very highly valued by a large circle of friends. He married, in 1862, a daughter of the late A. Follett Osler, F.R.S., of Edgbaston. He died, November 12, 1909, at his house at Ringwood, leaving one son and one daughter.

In the preparation of this notice, use has, by permission, been made of an article published in 'Nature,' vol. 82, p. 101 (Nov. 25, 1909).

G. C. F.

SIMON NEWCOMB, 1835—1909.

SIMON NEWCOMB was born in Nova Scotia in 1835, at Wallace, a pretty village at the mouth of the river of that name. His father was a country school teacher, a nomadic profession in a thinly-populated district. He was the most rational and most dispassionate of men. Newcomb in his autobiography (which will be freely quoted in this notice) tells us that his father had learned from careful study "that the age at which a man should marry was twenty-five. A healthy and well-endowed offspring should be one of the main objects in view in entering the marriage state, and this required a mentally-gifted wife. She must be of different temperament from his own, and an economical housekeeper. So when he found the age of twenty-five approaching he began to look about. There was no one in Wallace who satisfied the requirements.

"He therefore set out afoot to discover his ideal. In those days and regions the professional tramp and mendicant were unknown, and every farmhouse dispensed its hospitality with an Arcadian simplicity little known in our times. Wherever he stopped overnight he made a critical investigation of the housekeeping, perhaps rising before the family for this purpose. He searched in vain until his road carried him out of the province. One young woman spoiled any possible chance she might have had by lack of economy in making the bread. She was asked what she did with an unnecessarily large remnant of dough which she left sticking to the sides of the pan. She replied that she fed it to the horses. Her case received no further consideration.

"The search had extended nearly a hundred miles when, early one evening, he reached what was then the small village of Moncton. He was attracted by the strains of music from a church, went into it, and found a religious meeting in progress. His eye was at once arrested by the face and head of a young woman playing on a melodeon, who was leading the singing. He sat in such a position that he could carefully scan her face and movements. As he continued this study, the conviction grew upon him that here was the object of his search. That such should have occurred before there was any opportunity to inspect the dough-pan may lead the reader to conclusions of his own. He inquired her name—Emily Prince. He cultivated her acquaintance, paid his addresses, and was accepted." "My mother was the most profoundly and sincerely religious woman with whom I was ever intimately acquainted, and my father always entertained and expressed the highest admiration for her mental gifts, to which he attributed whatever talents his children might have possessed. The unfitness of her environment to her constitution is the saddest memory of my childhood. More I do not trust myself to say to the public, nor will the reader expect more of me."

His early years were passed amid social conditions of the utmost simplicity.

"The women sheared the sheep and made the clothes, but any man who allowed wife or daughter to engage in heavy work outside the house would have lost caste."

As a child, Newcomb was precocious in arithmetic, doing extraordinary calculations for his years with the assistance of a napped counterpane.

He was never known to deviate from the truth in one single instance either in infancy or youth. This high praise comes from his father, who adds a little later: "You were uncommonly deficient in that sort of courage necessary to perform bodily labour. Until nine or ten years of age you made a most pitiful attempt at any sort of bodily or, rather, handy work."

He was an omnivorous reader, and a very careful one, never passing a word that he did not understand.

Among his neighbours he acquired a reputation for learning that he felt was not appreciated, while he was painfully conscious of his inability to drive oxen. And he says: "My boyhood was, on the whole, one of sadness." At the age of sixteen Newcomb almost decided upon the trade of a carpenter, but at the last moment he was apprenticed for five years to a certain Doctor Foshay, who turned out to be a quack. While he was with the doctor: "A book peddler going his rounds offered a collection of miscellaneous books at auction. I bought, among others, a Latin and a Greek grammar, and assiduously commenced their study. With the first I was as successful as could be expected under the circumstances, but failed with the Greek, owing to the unfamiliarity of the alphabet, which seemed to be an obstacle to memory of the words and forms."

At the end of two years he ran away and worked his passage on board ship to Salem.

The year 1854 was spent as teacher in a country school. In 1855 he got a better position of the same character at Sudlersville.

The next year he taught in the family of a planter named Bryan, some fifteen or twenty miles from Washington. His first visit to the capital had been in 1854, but now they became frequent. In 1856, in the Smithsonian Library he first saw Laplace's "Mécanique Céleste." "About December, 1856, I received a note from [Mr. J. E. Hilgard, assistant in charge of the Coast Survey Office], stating that he had been talking about me to Prof. Winlock, Superintendent of the 'Nautical Almanac,' and that I might possibly get employment on that work. When I saw him again I told him that I had not yet acquired such a knowledge of physical astronomy as would be necessary for the calculations in question; but he assured me that this was no drawback, as formulæ for all the computations would be supplied me. I was far from satisfied at the prospect of doing nothing more than making routine calculations with formulæ prepared by others; indeed, it was almost a disappointment to find that I was considered qualified for such a place. I could only console myself by the reflection that the ease of the work would not hinder me from working my way up." The result was that one frosty morning in January, 1857, he took his seat in the office of the "Nautical

Almanac," at Cambridge, Mass. He was then in his twenty-second year. From this time onwards his career was one of unchequered brilliancy. In 1860, he went on an eclipse expedition up the Saskatchewan River. The weather was cloudy and nothing was seen of the eclipse.

In 1861, he was appointed Professor of Mathematics in the United States Navy, and as such he commenced transit instrument work at Washington Observatory on October 7.

Although he had been on an eclipse expedition in the previous year, he had never been inside an observatory, except on two or three occasions at Cambridge as a visitor. In September, 1863, he took charge of the mural circle. At this time it was usual at Washington for each observer to reduce his own observations. Newcomb contrived to introduce a uniform system of reduction in imitation of the system already introduced by Airy at Greenwich.

In October, 1865, the new transit circle arrived from Berlin. In the following years Newcomb succeeded in eradicating a vicious practice that obtained not only at his own observatory, but all over the world. He pointed out that clock stars ought only to be kept for place when at least a twelve-hour group has been obtained. For if an error depending on the sine or cosine of the right ascension exists in the clock star list, and observations only extend over six hours, the same error will be reproduced with only an infinitesimal degree of damping; whereas with twelve-hour groups the error is quickly damped out. In 1869, he observed an eclipse in Iowa, and in that year he began to turn his earnest attention to the problems presented by the moon's motion.

In 1870 he visited Europe for the first time, partly for an eclipse at Gibraltar that was obscured by cloud, and partly to search through the records of various observatories for seventeenth century observations of the moon.

In 1875 Newcomb was offered and declined the directorship of Harvard Observatory. On September 15, 1877, he took charge of the Nautical Almanac Office, a post which he held until his retirement in 1897.

The beginning of Newcomb's astronomical career coincided with the publication of Hansen's tables of the moon. These tables were an enormous advance on those previously in existence. Hitherto errors in computed coefficients had in many instances exceeded two or even three seconds of arc. Hansen's tables contain two or three errors in computed coefficients exceeding half a second of arc, but as a rule he attains a far greater accuracy.

The chief defect of the tables lies in the determination of the arbitrary constants, and in the omission of a whole group of planetary terms, the existence of which was not then suspected. A passage in Newcomb's autobiography throws much light on the cause of the former defect.

Hansen worked with the assistance of one computer only; this was no hardship while he was engaged on his theory. It would, in fact, require some management to assign work to a much larger staff simultaneously.

But when he came to compare his theory with observation and to determine his arbitrary constants, he was exceedingly short-handed. Instead, therefore, of making a detailed comparison with all existing observations, he based his comparison on a few years only. The result was utterly unworthy of his great theory. His parallactic coefficient is two seconds in error, his principal elliptic term half a second in error, and so on. He also postulated a mechanical ellipticity in the moon's figure at least four times too large. With all these defects his tables mark an enormous advance, and his contemporaries believed that "our troublesome satellite has been at length reduced to order."

At the end of Newcomb's career the theory of E. W. Brown, which is to replace Hansen's, is complete, and tables based upon it are in the course of preparation. The advance upon Hansen will be greater than Hansen's advance upon his predecessors, and yet no one believes that the problem of the moon is solved.

Newcomb was in touch with all the work done in this period of fifty years, and great portions of this work were done by himself. His first investigation connected with the moon was a redetermination of the elliptic elements of the moon's orbit.

In this paper he brought to light an empirical term that is now known as the Jupiter evection term. It manifested itself as a fluctuation in the moon's eccentricity and perigee with a period of seventeen years. Some years earlier Airy had analysed eighty years of observations, and had been almost within touch of this term, but had wrongly identified the period as that of the moon's node, nineteen years.

Newcomb at this time did the bulk of his own computing, and to this fact his superior success is plainly due.

The explanation of the term was quickly assigned by Nevill to the action of Jupiter. At a time when most astronomers hardly realised that Hansen's tables needed correction, Nevill was being the pioneer in a new branch of the lunar theory.

The question of planetary inequalities was subsequently taken up by G. W. Hill, by Radau, by Newcomb himself and by E. W. Brown. It may now be considered as worked out. At this point we may notice one other contribution of Newcomb's to the gravitational theory of the moon, viz.: a beautiful theorem for obtaining the secular accelerations resulting from the secular diminution of the eccentricity of the earth's orbit round the sun.

Hansen, like Laplace, had assigned 12 seconds as the secular acceleration of the moon's mean motion; Adams had shown that this quantity was twice too large; but Adams' accuracy was not immediately admitted.

Some years later Newcomb produced his theorem, and again quite recently E. W. Brown, with the help of Newcomb's theorem, has practically reproduced Adams' value, which by that time was generally accepted, in a paper so short and simple that one wonders how there could ever have been any controversy on the subject.

We turn now to Newcomb's work of comparison of observations with theory.

His great work entitled "Researches on the Motion of the Moon" secured for its author the Copley Medal of the Royal Society.

In the first section he considers the ancient and mediæval eclipses. He rejects the solar eclipses, he ignores the magnitudes of lunar eclipses; and he shows the times of the lunar eclipses to be fairly consistent among themselves and with a secular acceleration slightly greater than the theoretical.

He assigns the excess to tidal friction. This no doubt is a *vera causa*, but there is at present no independent measure of its magnitude. In the concluding part of the "Researches" he gives the results of occultations observed in the century preceding 1750. These occultations were not only worked up by Newcomb, but actually extracted by him from the archives of European observatories. He has since extended his series of occultations down to 1898 and the results were published early this year. The older occultations required immense diligence. The observers' hieroglyphics had to be collected in many cases, in order to decipher their meaning. Clock errors had to be obtained in any way that was possible. Finally taking all the other quantities as known, a somewhat rough determination of the moon's mean error of longitude is obtained. From that it appears that in the moon's observed motion there exists a term unknown to theory with a period of about three hundred years.

To Newcomb, and to him alone, we owe such knowledge as we have of the moon's motion in the century preceding 1750. In his autobiography, Newcomb says: "One curious result of this work is that the longitude of the moon may now be said to be known with greater accuracy through the last quarter of the seventeenth century than during the ninety years from 1750 to 1840." The reductions for the latter period leave very much to be desired, but Newcomb's remark is too drastic. For instance, Newcomb has with his occulations traced an empirical term of sixty years' period back to 1820; before that date it is lost in the accidental error of his material. That term can be traced in the meridian observations back to 1750.

Newcomb's 'Researches' also contain the first recognition of the error of Hansen's mean motion of the moon's node. He deduces his correction by a comparison of an eclipse of 1715 with transit observations in or about 1868. Although the time interval is large, the position of the node on the first occasion is subject to much uncertainty.

The exact measurement of this motion is of great interest in view of the discrepancy from theory exhibited by the perihelion of Mercury.

Turning now to planetary theory, Newcomb's first paper was an investigation on the orbits of minor planets, with the object of ascertaining whether an explosion of a single planet could be assigned as their origin.

If such an explosion really took place, and if all secular changes affecting asteroids were already recognised, it would be possible to assign the place and time of the catastrophe; and the date, if obtained at all, would be obtained with an exactness unparalleled in other speculations as to the past

history of the universe. Unfortunately, Newcomb's conclusions were negative.

At this time Leverrier was still working out his theory of the larger planets, going outwards from the sun. He had not yet reached Uranus and Neptune, so Newcomb took up the orbits of these two planets, and also of their satellites, in order to determine their masses.

He also made a series of observations for this special purpose, and his work was rewarded with the Gold Medal of the Royal Astronomical Society.

Before the close of his life Newcomb had constructed tables for all the larger planets, and in addition for the minor planet Polyhymnia, in order to determine the mass of Jupiter. G. W. Hill relieved him of "about the most difficult [part] in the whole work—the theory of Jupiter and Saturn. Owing to the great mass of these 'giant planets,' the inequalities of their motion, especially in the case of Saturn, affected by the attraction of Jupiter, are greater than in the case of the other planets.

"Leverrier failed to attain the necessary exactness in his investigation of their motion. . . . [G. W. Hill] laboured almost incessantly for about ten years when he handed in his manuscript of what now forms Volume IV of the 'Astronomical Papers.'"

Newcomb followed Leverrier's methods in essentials. "Two systems of computing planetary perturbations had been used, one by Leverrier, while the other was invented by Hansen. The former method was, in principle, of great simplicity, while the latter seemed to be very complex and even clumsy. I naturally supposed that the man who computed the direction of the planet Neptune before its existence was known must be a master of the whole subject, and followed the lines he indicated.

"I gradually discovered the contrary, and introduced modified methods, but did not entirely break away from the old trammels.

"Hill had never been bound by them, and used Hansen's method from the beginning. Had he given me a few demonstrations of its advantages I should have been saved a great deal of time and labour."

Possibly in order that his own work, regarded as a verification of Leverrier's, might be quite independent, Newcomb introduced some changes into the calculations.

Leverrier, for instance, used the mean longitudes of the planets. Newcomb used the mean anomalies. Leverrier develops algebraically, according to the mean angles, and then reduces to arithmetical values. Newcomb develops algebraically in eccentric anomalies, reduces to number, and then transforms to mean anomalies arithmetically in each separate case. In a later volume he gives algebraic formulæ for the mean anomalies. He has enormously improved Leverrier's notation by introducing an operator that he terms "D."

When the final perturbations are compared, we are struck by how little Leverrier left for his successors. When allowance is made for the difference in the assumed masses of the planets, the difference in the perturbations calculated by Leverrier and Newcomb respectively are not such as could be

detected by observation. Newcomb has, however, improved the arbitrary constants, he has used the same planetary masses throughout all the tables, and finally he has enormously reduced the labour required for an ephemeris by following the methods used by Hansen for the moon.

Like the lunar theory, the planetary theory is not yet perfect. The principal outstanding problems are:—

(i) A centennial motion of forty seconds in the perihelion of Mercury, and
(ii) the orbit of Mars.

Since Leverrier's time, the adopted solar parallax has increased by nearly three per cent. and consequently the mass of the earth by eight per cent.

Mars is the planet whose tabular orbit is most affected by an erroneous mass of the earth; but although Newcomb was able to avail himself of the more accurate value of the mass of the earth, his tables of Mars are far less satisfactory than any other of his tables, and the problem has not yet been solved.

The discordance from theory in the motion of the perihelion of Mercury had been discovered by Leverrier from a discussion of the transits of Mercury.

Newcomb went over the ground again, with a little added material, and asking an additional question, "Are the errors of Mercury so related to those of the moon as to suggest that the earth is not a perfect timekeeper?" he found that he was not able to assert that such a relation existed.

Newcomb's Fundamental Catalogue and his "Astronomical Constants" must be mentioned, as well as his determination of Precession. These are great works in themselves, but to Newcomb mainly incidents in the thorough discussion of the motion of the moon and of the planets.

It was Newcomb also who assigned the lengthened period of latitude-variation to want of rigidity in the earth.

Newcomb visited Europe for the last time in 1908. Soon after his return his friends heard that he was hopelessly ill. He still continued his interest in his work, and passed through the press his last paper on the Moon.

He died on July 16, 1909, at the age of seventy-four. Twenty-two years had he spent in darkness before he became an astronomer, and subsequently the congenial nature of his work made the world for him one of "sweetness and light."

P. H. C.